Using this concept of definition, I compared my nephanalysis with those presented by the authors and the probability-of-cloudiness charts (diagnostic charts). The results are quite interesting.

On page 182, the authors note a "discrepancy" between the diagnosed and the observed clouds south of Iceland on 1500 GMT February 2 (fig. 10b in their paper). They are quite correct in rejecting this cloud layer since they observed only stratocumulus on the videograph. Had they initially rejected these clouds, which form *behind* the trough as a result of the flow of cold air over warm water, this discrepancy would not have occurred at all. These stratocumulus, stratus, or cumulus clouds are easily identified on the satellite cloud pictures. Their rejection in the nephanalysis of significant clouds would help greatly toward eliminating discrepancies.

More important because of their definition, the authors missed identifying a rather significant cloud system in the nephanalysis they used in figure 12a and as a result had an apparent discrepancy that in fact did not exist at all. In my nephanalysis, there was an extensive cloud system in practically the exact area where their method diagnosed one to exist! This is shown in my figure 1. This system existed in the generation region for many days, and its shape suggests cyclonic flow. The authors' method succeeded where they thought it had failed.

Further, my nephanalysis does show a breakup of the clouds over the Texas-Mexican border and eastward, nearly as they show on their probability-of-analysis chart! The nephanalysis they used did not show this. Again their method succeeded.

A cloud system which the authors successfully diagnosed with their method, but were surprised to find agreement with, was one which appeared over Mexico. This is the system over the Baja California region which they discuss on page 182 and depict in figure 12. This is also shown in my figure 1. This system was associated with a trough that had advanced across the Pacific, and which became quite weak as it crossed the Rocky Mountains. The clouds associated with this trough became quite weak north of Mexico, while the clouds maintained some identity while crossing Mexico. The system over Mexico, associated with this system, later appeared as a large cloud mass over the western Gulf and Texas coast on February 3, 1965.

Comparisons that I made with the other figures in the article verify that the authors have very definitely diagnosed major cloud band systems more adequately than they thought. They simply used an inadequate basis for defining significant major cloud band systems. I do think they would find it quite fascinating to use some of the Nimbus, ESSA satellite, and ATS data in testing their method in connection with their probability-of-cloudiness method.

REFERENCES

1. R. E. Nagle, J. R. Clark, M. M. Holl, and C. A. Riegel, "Formulation and Testing of a Program for the Objective Assembly of Meteorological Satellite Cloud Observations," *Monthly Weather Review*, vol. 95, No. 4, Apr. 1967, pp. 171–187.
2. B. F. Watson, "Global Cloud Systems," submitted for publication, 1966.

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Reply

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We wish to thank Mr. Watson for his interest in our paper. The essence of his comments concerns the adequacy of our definition and analyses of regions of synoptically significant layer cloudiness. Mr. Watson's definition is undoubtedly more precise (synoptically) than ours and its use in governing the analyses might have produced better correspondences between the observed and diagnosed cloudiness. However, his suggested procedure suffers from the same inherent limitations as ours in that it requires the subjective interpretation of the cloud patterns viewed in the videographs. This points out the need for an automated method of delineating areas of synoptically significant layer cloudiness. Recent developments indicate that such a procedure is now feasable and that, if desired, the cloud information could be assimilated into the Program in a more objective manner.

In this regard, Fritz [1] has shown that cloud albedo is a function of droplet size-and-number distribution, geometric thickness, and sun zenith angle; therefore, synoptically significant layer cloudiness, having a relatively high liquid water content, should also have a high albedo. This has been subjectively confirmed, as evidenced by the bright-appearing cloud bands viewed in the satellite videographs. Brightness is not a necessary and sufficient condition for delineating such cloud areas, as clouds which are not associated with large-scale lifting may also be highly reflective (for example, coastal stratus and stratocumulus clouds). However, the clouds of interest are also usually characterized by their great depth and therefore by their relatively cold tops. Cloud-top temperatures could be used as a further distinguishing criterion for excluding clouds which are not associated with large-scale lifting.

Quantitative measures of both cloud brightness and cloud-top temperatures can be readily obtained from radiometer measurements in appropriate spectral regions [2]. The use of these data in the current context would require the derivation of suitable functional relationships among cloud-top temperatures, cloud albedo, and the occurrence of layer cloudiness. The feasibility of this approach has been demonstrated by Lethbridge [3], who reported a high, joint correlation among cloud-top temperatures, cloud reflectance, and precipitation amount. The resolution of either the medium or high resolution radiometers currently in use appears to be more than sufficient to allow spatial definition of the cloud areas of interest. Thus, it appears possible that synoptically significant layer cloudiness analyses could be produced by the computer processing of radiometer observations.

Overriding the feasibility of this approach, however, is my belief that the input of cloud information into an operational system would be redundant. The results of our Experiment No. 4 clearly indicate that in regard to the diagnosis of layer cloudiness, the initial moisture distribution is relatively unimportant in comparison to the net-vertical displacement the air parcels experienced during their prior histories. The accuracy of the diagnosed three-dimensional displacement of the parcels is, of course, strongly dependent upon the accuracy of the mass structure *evolution,* and therefore the most pertinent direction for exploiting satellite data in an objective weather analysis system is in improving the mass-structure analyses. This direction was explored in another aspect of our research and a method for incorporating these data into an objective analysis system was outlined [4].

REFERENCES

1. S. Fritz, "Scattering of Solar Energy by Clouds of 'Large Drops'," *Journal of Meteorology,* vol. 11, No. 4, Aug. 1954, pp. 291–300.
2. W. R. Bandeen, R. A. Hanel, J. Licht, R. A. Stampfl, and W.G Stroud, "Infrared and Reflected Solar Radiation Measurements from the TIROS II Meteorological Satellite," *Journal of Geophysical Research,* vol. 66, No. 10, Oct. 1961, pp. 3169–3185.
3. M. Lethbridge, "Precipitation Probability and Satellite Radiation Data," *Monthly Weather Review,* vol. 95, No. 7, July 1967, pp. 487–490.
4. R. E. Nagle, J. R. Clark, M. M. Holl, and C. A. Riegel, "Objective Assembly of Meteorological Satellite Data," *Final Report,* Contract No. N62306–1775, Meteorology International Inc., Monterey, Calif., Aug. 1966.

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